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# Vertical Transport by Small Scale Stratospheric Turbulence: A Critical Review

EDMOND M. DEWAN

17 February 1981

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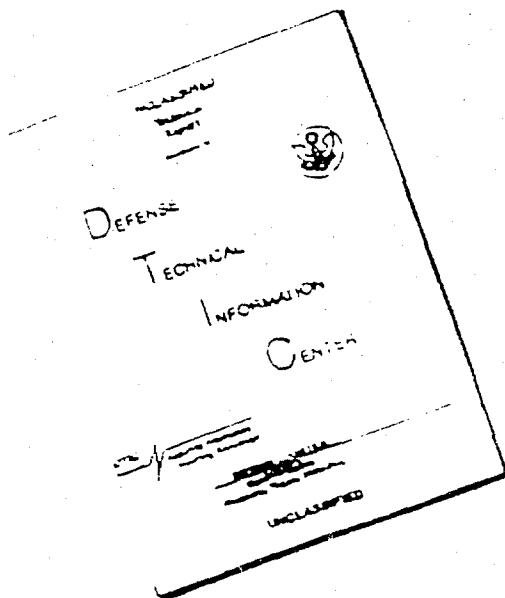
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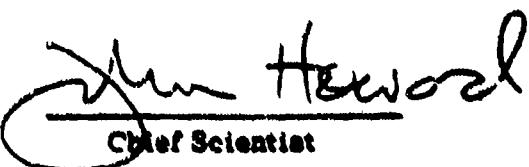


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20. Abstract (Continued)

questions are exhibited. The conclusion is reached that more experimental work needs to be done before one has a reliable estimate for  $K_B$ .

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## Vertical Transport by Small Scale Stratospheric Turbulence: A Critical Review

### 1. INTRODUCTION

The meaning of the word "turbulence" is ambiguous. On one hand, it could refer to large scale synoptic motions that take place on a global scale. In this connection the phrase "two dimensional turbulence" has been used. On the other hand, it more frequently refers to small scale three-dimensional chaotic motion which causes intimate mixing on a small scale. Similarly the term "eddy diffusion coefficient," which implies a pseudo-diffusion effect due to the eddy flow, can be used in more than one manner. On one hand, it can include large scale synoptic effects (which are most often regarded as advective in nature) together with small scale turbulence effects. On the other hand, it can refer exclusively to small scale three-dimensional turbulence effects.<sup>1</sup> As can be seen by the title of this review, only the latter type of "diffusion" will be considered here.

The stratosphere, by definition, is an exceptionally stable part of the earth's atmosphere. Turbulence in such a stable fluid has a certain peculiarity of structure which must not be ignored. It occurs in relatively thin layers separated by what are usually large layers of essentially laminar flow. This is true not only

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(Received for publication 17 February 1981)

1. Tennekes, H. and Lumley, J.L. (1972) A First Course in Turbulence, The MIT Press, Cambridge, MA.

for the stratosphere but for the upper ocean as well.<sup>2-4</sup> In addition, it is also sometimes true for the troposphere.

The word "blini" (pancakes) is sometimes used to describe this layered structure of stratified turbulence in the atmosphere.<sup>5</sup> Such clear air turbulence (CAT) pancakes are of the order of 100 m thick and 10 km in the horizontal direction. They are due to the shear or Kelvin-Helmholtz instability. These layers are always assumed to occur at random heights and times with random thicknesses. They are not only intermittent but presumably, also rare (of the order of one percent of the fluid volume).<sup>6-12</sup>

Let  $K_B$  denote the effective diffusivity for stratified (small scale) transport over large regions of altitude. The subscript B stands for "bulk" as opposed to the local eddy diffusivity that might be encountered within an active turbulent layer. This parameter is the one upon which we will focus our attention. The two main questions which need to be answered are: (1) How important is the role played by small scale turbulence in the overall vertical transport of tracers in the stratosphere; and (2) What is its approximate value?

The practical importance of these questions derives, of course, from the stratospheric pollution problem. As is well known, the possibility exists that

2. Woods, J. D. (1968) Wave-induced shear instability in the summer thermocline, J. Fluid Mech., 32:791-800.
3. Woods, J. D. and Willey, R. L. (1972) Biflow turbulence and ocean microstructure, Deep Sea Research and Oceanic Abst., 19:87-121.
4. Phillips, O. M. (1967) On the Bolgiano and Lumley-Shur Theories of the Buoyancy Subrange, Atmospheric Turbulence and Radio Wave Propagation, A. M. Yaglom and V. I. Tatarsky, Eds., Nauka, Moscow.
5. Bretherton, F. P. (1969) Waves and turbulence in stably stratified fluids, Radio Science 4:1279-1287.
6. Stewart, R. W. (1969) Turbulence and waves in a stratified atmosphere, Radio Sci. 4:1269-1278.
7. Pao, Y-H. and Goldburg, A. (1969) Clear Air Turbulence and Its Detection, Plenum Press.
8. Rosenberg, N. W. and Dewan, E. M. (1975) Stratospheric Turbulence and Vertical Effective Diffusion Coefficients, AFCLR-TR-75-0510, AD A019 708.
9. Dewan, E. M. (1979) Estimates of Vertical Eddy Diffusion Due to Turbulent Layers in the Stratosphere, AFGL-TR-79-0042, AD A069 750.
10. Dewan, E. M. (1979) Stratospheric spectra resembling turbulence, Science 204:832-835.
11. Dewan, E. M. (1980) A One-Dimensional Vertical Diffusion Parameter for Extremely Inhomogeneous, Layered Turbulence In Stratified Fluids, AFGL-TR-80-0186, AD A090 340.
12. Dewan, E. M. (1981) Turbulence vertical transport due to thin intermittent mixing layers in the stratosphere and other stable fluids, Science 211:1041-1042.

oxides of nitrogen ( $\text{NO}_x$ ) and chlorofluorocarbons can catalytically destroy ozone and, hence, cause global problems due to the resulting enhanced transmission of solar ultraviolet radiation to the earth's surface. (Some of the possible disastrous consequences are described in CIAP Monograph 1, 1975.)<sup>13</sup>

As Reiter<sup>14</sup> has pointed out, there are several mechanisms for vertical transport associated with the stratosphere. His paper, which concerns the stratospheric-tropospheric exchange processes (not necessarily transport throughout the volume of the stratosphere) lists the following in order of importance: (1) Hadley cell motion (38 percent), (2) large scale eddies of the scale of cyclones and anticyclones (20 percent), and (3) seasonal tropospheric height changes (10 percent). The percents refer to the fraction of the mass equivalent to one hemisphere stratosphere transferred through the tropopause in a year. These all add up to values which are consistent with "residence times" of materials deposited in the stratosphere such as atomic bomb debris. The "fallout times" or "resident times," of course, measure the effects of all processes simultaneously. Reiter<sup>14</sup> also states that, with regard to stratospheric-tropospheric exchange, turbulence plays an apparently insignificant role. Whether or not such a conclusion is correct, it does not answer the question of whether or not small scale turbulence within the volume of the stratosphere is significant for vertical transport there.

This raises an interesting question: If fallout times are known to some degree, why is it important to know the details of the removal process and, in particular, the relative significance of the role of turbulence? One answer is that one must understand the mechanism of transport if one is ever to estimate the effects of large perturbations in stratospheric composition. These could be caused by long-term gradual changes due to pollution or by short-term catastrophic changes due to large nuclear effects or rare, natural, large perturbations. Such large chemical changes could completely change the dynamics of the stratosphere and, hence, the residence times. After all, in spite of the fact that the stratosphere is defined in terms of its dynamic stability, it is in fact its composition (that is, the ozone) which causes the stability to exist. The ozone, by absorbing ultraviolet light, causes the temperature inversion which is, in effect, the stratosphere. This stability, in turn, enables large amounts of ozone to accumulate without too much loss. The ozone, in effect, has created its own container. Its depletion would also deplete the container! Thus, composition affects dynamics, and a model which takes all the important mechanisms and these mutual

13. CIAP (1975) The Natural Stratosphere of 1974 CIAP Monograph 1,  
DOT-TST-75-51.

14. Reiter, E. R. (1975) Stratospheric-tropospheric exchange processes,  
Rev. Geophys. and Space Phys. 13:459-474.

interactions into account would be needed to estimate the impact and subsequent effects following a large perturbation.

There is a second reason for the necessity of knowing the value of  $K_B$ . This relates to stratospheric chemistry in general. Chemicals cannot react until they are mixed into intimate contact. Turbulence of the small scale variety is the only mechanism that can bring this about. It is well known that the vertical profile of any stratospheric constituent is highly jagged and layered. This indicates that vertical mixing takes place over small vertical scales and also that it is erratic.

One of the most important objectives of this review is to reveal the high level of ignorance which surrounds the value of  $K_B$ , the effective turbulence diffusion parameter. Unfortunately, the magnitude of ignorance is not generally appreciated. The majority of writers<sup>15-18</sup> seem to regard the issue as relatively well in hand and that the order of magnitude of  $K_B$  (globally averaged) is 0.01 m<sup>2</sup>/s. This is to be compared to the overall "eddy diffusivity" based on fallout times and due to all processes which are 10 to 100 times larger.<sup>18-21</sup> It will be shown, however, that the estimate of  $K_B \sim 0.01$  m<sup>2</sup>/s rests on very questionable foundations. While this does not mean that this estimate is not correct in principle, it does mean that more experiments will have to be performed before one can accept it as scientifically valid.

Our plan is primarily to review the techniques by which  $K_B$  is estimated and to examine the assumptions (stated and otherwise) which are involved. As will be seen, there are a number of unanswered questions that are thus exposed.

In more detail, we shall do the following. First, we shall consider the work of Lilly, et al.<sup>15, 16</sup> They estimated the value of the local eddy diffusion

- 
15. Lilly, D. K., Waco, D. E., and Adelfang, S. I. (1974) Stratospheric mixing estimated from high-altitude turbulence measurements, J. Appl. Met., 13:488-493.
  16. Lilly, D., Waco, D., and Adelfang, S. (1975) Stratospheric mixing estimated from high-altitude turbulence measurements by using energy budget techniques, The Natural Stratosphere of 1974, CIAP Monograph 1, DOT-TST-75-31:6-81 to 6-90.
  17. Heck, W. and Panofsky, H. A. (1975) Stratospheric mixing estimates from heat flux measurements, The Natural Stratosphere of 1974, CIAP Monograph 1, DOT-TST-75-61:6-90 to 6-92.
  18. Mahlman, J. D. and Moxim, W. J. (1978) Tracer simulation using a global general simulation model . . ., J. of the Atm. Sci., 35:1340-1374.
  19. Junge, C. E. (1963) Air Chemistry and Radioactivity, Academic Press, New York, 250.
  20. Bauer, E. (1975) Dispersion of tracers in the atmosphere, The Natural Stratosphere of 1974, CIAP Monograph 1, Final Report, DOT-TST-75-51, 6-39 to 6-55.
  21. Luther, F. M. (1975) Large-Scale Eddy Diffusion, The Natural Stratosphere of 1974, CIAP Monograph 1, Final Report, DOT-TST-75-51.

coefficient,  $K_H$  ( $H$  for heat transfer), from power density spectra of wind fluctuations. In contrast to  $K_B$ ,  $K_H$  refers to the diffusion of a passive scalar such as heat that is caused by the active turbulence within a plume. The wind fluctuation measurements were obtained from the published reports of Crooks et al,<sup>22</sup> which gave in-situ measurements obtained by means of an instrumented U-2 aircraft flying in the stratosphere. The spectra were first used to estimate  $\epsilon$ , the viscous dissipation rate. The latter was used to obtain  $K_H$ . As mentioned, when they then estimated  $K_B$  from  $K_H$ , they obtained  $0.01 \text{ m}^2/\text{s}$  as the order of magnitude. Next, we will consider the work of Panofsky and Heck,<sup>17</sup> They obtained experimental evidence based on the HICAT data which supported the technique of Lilly, et al<sup>16</sup> for the estimation of  $K_H$  from  $\epsilon$ . The work of Zimmerman and Loving<sup>23</sup> is discussed next. They, too, obtained  $\epsilon$  from the HICAT spectra,<sup>22</sup> but their method for estimating  $K_H$  from  $\epsilon$  differed from the method of Panofsky and Lilly.<sup>15-17</sup> Their values for  $K_H$  (and by implication, using the reasoning of Lilly, et al,  $K_B$ )<sup>15, 16</sup> were, by comparison, enormous.

After this a general, critical discussion is given of some of the basic assumptions made by the above authors. In this discussion, several unanswered questions are raised. The work of Rosenberg and Dewan,<sup>8</sup> is then reviewed. They estimated  $K_B \sim 0.3 \text{ m}^2/\text{s}$ , a value in between those of Lilly et al,<sup>15, 16</sup> and Zimmerman et al.<sup>23</sup> The unanswered questions raised by this work are pointed out. Finally, in the conclusion, the impact of the work of Mahlman et al,<sup>18</sup> on global circulation simulations is pointed out. Two experiments are given in the text which are the most crucial ones to perform at this time in order to permit, perhaps for the first time, a reasonably dependable estimate of  $K_B$ .

## 2. THE WORK OF LILLY, WACO, AND ADELFANG

Project HICAT (High Altitude Critical Atmospheric Turbulence) involved 286 flights of U-2 aircraft in the 14 to 21 km altitude range of the stratosphere. The "turbulent patches" included only 2 to 5.2 percent of the total flight distances and were highly correlated with categories of terrain. As was mentioned above, the power density spectra obtained from these data were used to obtain the viscous dissipation rate,  $\epsilon$ , and  $K_H$  by these authors.

- 
22. Crooks, W. M., Hoblit, F. M., and Prophet, D. T., et al (1967) Project HICAT: An Investigation of High Altitude Clear Air Turbulence, Tech. Rept., see also AD 846 086, AD 847 497 (1968).
  23. Zimmerman, S. P. and Loving, N. (1975) Turbulent dissipation and diffusivities in the stratosphere using Richardson's technique, The Natural Stratosphere of 1974, CIAP Monograph 1, Final Report, DOT-CIAP, DOT-TST-75-51.

To obtain  $\epsilon$ , they noted that the log-log plots of the spectra had a nearly  $-5/3$  slope, and hence they assumed that this implied that they were observing an "inertial range" spectrum. As is well known, Kolmogoroff<sup>24</sup> predicted this slope on the basis of a similarity argument, and it has subsequently been amply verified experimentally. (It is frequently observed in geophysical flows.) Thus, these authors used the Kolmogoroff relation

$$\phi(k) = a \epsilon^{2/3} k^{-5/3} \quad (1)$$

(where  $\phi(k)$  is the one-dimensional velocity fluctuation power density spectrum,  $k$  the wave number, and where  $a$  is the constant of order unity derived from experiment). Solving Eq. (1) for  $\epsilon$  gives its value in terms of the spectrum,

More specifically, they integrated Eq. (1) from the value of  $k$  corresponding to wavelengths equal to  $(610 \text{ m})^{-1}$  or  $(2,000 \text{ ft})^{-1}$  to  $k = \infty$ . Solving the resulting equation for  $\epsilon$  and using the fact that the integral of  $\phi(k)$  over  $k$  is equal to the mean square fluctuation velocity,  $\bar{v}^2$ , they obtained

$$\bar{\epsilon} = \left[ \frac{2}{3a_1} \right]^{3/2} \left( \bar{v}_1^2 \right)^{3/2} k_1 \quad (2)$$

where  $k_1 = (2\pi/610 \text{ m})$  and  $a_1$  is the constant of order unity which depends on  $i$  (the velocity component is designated by  $i$ ). In Eq. (2) the overbar denotes an average in the sense that one can use several spectra derived from a number of traverses through turbulence in order to estimate the average of  $\epsilon$ .

In order to estimate  $K_H$  from such estimates of  $\epsilon$  they made the following assumption:

$$P = B = \epsilon \quad (3)$$

where  $P$  means turbulent energy production, and  $B$  is the "up gradient buoyancy flux" or change of potential energy caused by mixing. More exactly

$$P = -\bar{v}'w' \frac{\partial \bar{v}}{\partial z} \quad (4)$$

$$B = -\bar{g} \frac{\partial \bar{w}'}{\partial z}$$

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24. Kolmogoroff, A. and Toppler, L. (1961) Turbulence, S. K. Friedlander, Ed., Interscience Publisher, NY.

where primed quantities are the turbulent deviations from the averages,  $g$  is the acceleration of gravity,  $\bar{\theta}$  the average potential temperature,  $\bar{V}$  the average horizontal velocity,  $w'$  the vertical velocity fluctuation,  $v'$  the horizontal velocity fluctuation,  $\theta'$  the potential temperature fluctuation, and  $Z$  is the vertical coordinate.

Next, they used the definition for  $K_H$  given by

$$\overline{w'\theta'} = -K_H \frac{\partial \bar{\theta}}{\partial Z} \quad (5)$$

and the corresponding definition for eddy viscosity (or momentum diffusivity)

$$\overline{v'w'} = -K_M \frac{\partial \bar{V}}{\partial Z} \quad (6)$$

in order to obtain

$$P = K_M \left( \frac{\partial \bar{V}}{\partial Z} \right)^2$$

$$B = K_H \left( \frac{g}{\bar{\theta}} \right) \frac{\partial \bar{\theta}}{\partial Z} \quad (7)$$

Since the buoyancy frequency,  $N_B$ , is defined by

$$N_B^2 = \left( \frac{g}{\bar{\theta}} \right) \frac{\partial \bar{\theta}}{\partial Z} \quad (8)$$

one obtains from Eqs. (7) and (3)

$$K_M S^2 - K_H N_B^2 = \epsilon \quad (9)$$

where  $S = \partial \bar{V} / \partial Z$ , the vertical shear of the average horizontal winds. The definition of the flux Richardson number,  $R_f$ , is

$$R_f = B/P = \frac{K_H N_B^2}{K_M S^2} \quad (10)$$

Hence Eq. (9) can be written

$$K_H N_B^2 \left( \frac{1}{R_f} - 1 \right) = \epsilon . \quad (11)$$

Citing the laboratory work of Thorpe,<sup>25</sup> they set  $R_f = 1/4$  and arrive at

$$K_H = \frac{\epsilon}{8N_B^2} . \quad (12)$$

They inserted values of  $\epsilon$  from Eq. (2) into Eq. (12) to get the estimates of  $K_H$  in their paper.

Since  $K_H$  refers only to the local value of eddy diffusivity within the thin actively turbulent layers, they then proceeded to estimate  $K_B$  from

$$K_B = F K_H \quad (13)$$

where  $F$  is the fraction of the vertical dimension occupied by the turbulent layers. The value of  $F$  was estimated by assuming\* it to be equal to the fraction of the U-2 flight trajectory that was "turbulent." This is of the order of a few percent. Taking suitable account of the variation of  $F$  with terrain category they concluded that  $K_B$ , averaged on a global scale, is  $0.012 \text{ m}^2/\text{s}$  to within (they claimed) one-half order of magnitude. This was estimated from values of  $K_H$  which ranged from 0.4 to  $1.14 \text{ m}^2/\text{s}$ . This is the value cited by Reiter<sup>14</sup> and Mahlman et al.,<sup>18</sup> in connection with small scale turbulence effects in the stratosphere and which seems to be the currently accepted estimate.

There is an unanswered question in connection with the assumption that the HICAT spectra are in the "inertial range" of length scales. This assumption seems to me to be in serious contradiction with certain experimental results published in the literature. Measurements were made *in-situ* by Crane,<sup>26</sup> Cadet<sup>27</sup> and Barat.<sup>28</sup> (See also Rosenberg and Dewan<sup>8</sup> and Stewart,<sup>6</sup>) Table 1 summarizes

\* Note, however, that the U-2 flew mostly horizontally whereas  $F$  refers to the vertical dimension.

25. Thorpe, S. A. (1972) Turbulence in Stably Stratified Fluids: A Review of Laboratory Experiments, IUCRM Colloquium on Boundary Layer Meteorology, San Diego, CA.
26. Crane, R. K. (1977) Stratospheric Turbulence Analysis, Air Force Geophysics Lab., Final Report, AFGL-TR-77-0207, AD 047 740.
27. Cadet, D. (1975) Vertical wind shear measurements in the lower stratosphere, Quart. J. R. Met. Soc. 101:485-493.
28. Barat, J. (1975) Etude experimentale de la structure du champ de turbulence dans la moyenne stratosphere, C. R. Acad. Sc. Paris 280(Serial B):881-893.

Table 1. Measurements of Turbulent Layer Thickness

Method	Authors	Turbulent Layer Thickness
Balloon-borne instruments	Barat <sup>28</sup>	200 m
	Anderson <sup>29</sup>	240 m
	Crane <sup>26</sup>	200 m
Rocket trail derived $R_1$ number profile	Rosenberg and Dewan <sup>8</sup>	80 m $\times$ 2
U-2 vertical traverse	Crane <sup>26</sup>	50 m (light to moderate)
	Crooks et al <sup>22</sup>	100 m (all)
		670 m $\pm$ 580 m

these measurements of turbulent layer thicknesses. All indicate that the order of magnitude of the turbulent layer thicknesses is 100 m. This would also be the outer scale presumably. The largest scales for the inertial range would be of the order of 1/10 of this<sup>30</sup> or 1/100.<sup>1</sup> In other words, the largest scales of the inertial range are expected to be 10 m (or even as small as 1 m). In contrast, the smallest scales of the HICAT spectra are about 50 m, the -5/3 slopes are seen out to 600 m routinely, and, on occasion, to beyond 10 km. A spectrum of -5/3 out to 5 km is not unusual. Such large scales are not only incompatible with the inertial range assumption, but the assumption of turbulent motion as well. Let us assume that the observations of Crane,<sup>26</sup> Cadet,<sup>27</sup> and Barat<sup>28</sup> are valid and not misleading in any fundamental way and that the large gap in scale cannot be explained in terms of aliasing effects (I would accept a factor of 5, but not 1000—see Gifford<sup>31</sup>) or other unexpected turbulence properties, for example, an anisotropic eddy shape of 1000:1, which has never been observed in the laboratory or in boundary layer turbulence. The conclusion then seems inescapable that HICAT spectra are predominantly due to gravity waves.<sup>10</sup> For this reason, all attempts to explain the HICAT spectra

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- 29. Anderson, A. D. (1957) Free air turbulence, *J. Meteorol.*, 14:477-494.
  - 30. Pond, S., Steward, R. W., and Burling, R. W. (1963) Turbulence spectra in the wind over waves, *J. Atm. Sci.*, 20:319-321.
  - 31. Gifford, F. (1959) The interpretation of meteorological spectra and correlations, *J. Meteorol.*, 16:344-346.

on the basis of any sort of turbulence phenomena must be regarded as invalid<sup>32,33</sup>. A quantitative attempt to explain the HICAT spectral shape in terms of gravity waves will be found in Dewan.<sup>10</sup>

### 3. THE WORK OF HECK AND PANOFSKY

The starting point for these authors is the definition of eddy diffusivity,  $K_H$ , in terms of heat flux

$$K_H = \frac{\text{FLUX}}{\frac{\partial \bar{\theta}}{\partial Z}} \quad (14)$$

that is,

$$K_H = \frac{-\bar{w}'\bar{\theta}'}{\partial \bar{\theta}/\partial Z} \quad (15)$$

which is Eq. (5). They depart from the direction of Lilly et al.,<sup>15,16</sup> and Zimmerman et al.,<sup>23</sup> by relating  $K_H$  to  $\bar{w}'\bar{\theta}'$  directly rather than to  $\epsilon$ . This value of  $\bar{w}'\bar{\theta}'$  is estimated directly from the HICAT data by integrating the cospectrum of  $w'$  and  $\theta'$ , that is, the real part of the cross power spectral density of  $\theta'$  and  $w'$ . In order to remove "random effects and the influence of gravity waves" they defined  $\bar{\theta}'w'$  as the area under the cospectrum for wavelengths less than 3000 m. Then they estimated  $K_H$  from Eq. (15). To compare these values of  $K_H$  with those obtained by Lilly et al.,<sup>15,16</sup> they computed  $\epsilon$  from the longitudinal wind component and plotted by  $K_H$  vs  $\log \epsilon$ . They found a perfect fit with  $K_H = \epsilon/3N_B^2$ . On this basis they claimed that  $K_H$  was of order  $10^4 \text{ cm}^2/\text{s}$  and with  $F \sim 0.01$  in Eq. (13),  $K_B \sim 0.01 \text{ m}^2/\text{s}$ . They thus fully supported the results given by Lilly et al.<sup>15,16</sup>

The large scale cut off of  $\lambda = 3000 \text{ m}$  exceeds what appears to be the outer length by a factor of 10. The choice of this wavelength is, therefore, somewhat arbitrary. Not only that, but it conflicts with the  $\lambda = 610 \text{ m}$  used by Lilly et al.,<sup>15,16</sup> If this work were to be extended, it would be extremely helpful if, in addition to the cospectra, the quadrature spectra were measured. (This is the imaginary part of the cross spectrum.) In this way one could make use of the excellent suggestion

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- 32. Dewan, E.M. (1976) Theoretical Explanation of Spectral Slopes in Stratospheric Turbulence Data and Implications for Vertical Transport, AFGL-TR-76-0247, AD A036 307.
  - 33. Weinstock, J. (1978) On the theory of turbulence in the buoyancy subrange of stably stratified flows, J. of the Atms. Sci., 35:634-649.

which was made by Stewart<sup>16</sup> and which was subsequently demonstrated by Axford.<sup>34</sup> This suggestion involved a technique for differentiating between turbulence and waves in the atmosphere. Defining  $\beta$  to be the phase angle:

$$\beta = \tan^{-1} \left( \frac{\text{Im } S_{w'g'}(k)}{\text{Re } S_{w'g'}(k)} \right) \quad (16)$$

(where  $S_{w'g'}$  is the cross spectrum between potential temperature fluctuation and vertical velocity fluctuation, Im designates the imaginary part and Re the real part) they found that one could distinguish between turbulence and waves as follows. When the flow is mainly turbulent one would expect  $\beta$  to be in the range  $+45^\circ$  to  $-45^\circ$  or  $135^\circ$  to  $225^\circ$ . For flow which is essentially wavelike,  $\beta = 90^\circ \pm 10^\circ$  (or  $270^\circ \pm 10^\circ$ ); and coherence values greater than 0.8 are usually found in a well-defined wave train.<sup>34</sup> If this were done, a less arbitrary criterion than ( $\lambda < 3000$  m) might be obtained. In my opinion it seems quite possible that the results and conclusions of Heck and Panofsky<sup>17</sup> could be significantly altered by such extensions of their work.

#### 4. THE WORK OF ZIMMERMAN AND LOVING

In a manner similar to the work previously cited, these authors assumed that the HICAT spectra represented inertial range turbulence and that one could use Eq. (1) for the purpose of estimating  $\epsilon$ . In order to estimate  $K_H$ , however, they proceeded quite differently and avoided the contradiction between Eqs. (1) and (3) that has been discussed. They used the work of Heisenberg<sup>35</sup> where a turbulent diffusivity was obtained by means of a dimensional argument, namely<sup>36</sup>

$$K_H(k_1) = C_1 \int_{k_1}^{\infty} \sqrt{\left(\frac{\phi(k)}{k^3}\right)} dk . \quad (17)^*$$

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\*There is an element of arbitrariness in Eq. (17) in the sense that any  $K$  of the form

$$K_H = (\text{constant}) \left[ \int_{k_1}^{\infty} \phi(k)^{s/2} k^{-(s+1)/2} dk \right]^{1/s}$$

or combination thereof will have the appropriate dimensions. Perhaps the best discussion of this subject will be found in Lin et al.<sup>37,38</sup>

Because of the large number of references cited above, they will not be listed here. See References, page 31.

From Eq. (1)

$$K_H(k_1) = a^{1/2} C_1 \epsilon^{1/3} k_1^{-4/3} . \quad (18)$$

These authors then addressed the question of what value to use for  $k_1$ . For this purpose they referenced a review by Phillips<sup>4</sup> which treated the "buoyancy subrange" of turbulence. In particular they used the work of Lumley<sup>39</sup> and Shur<sup>40</sup> whose theory predicts a -3 dependence of the spectrum upon  $k$  (that is,  $\phi \sim k^{-3}$ ) at scales significantly larger than the inertial range and a transition wave number,  $k_B$  given by

$$k_B = C \epsilon^{-1/2} N_B^{3/2} . \quad (19)$$

The so-called "buoyancy length," was given by  $l_B \sim k_B^{-1}$  and they estimated it to be in the range 15 m to 51 m. They chose  $C = 1$  arbitrarily in order to calculate this. They then chose  $k_1 = k_B$ . It should be noted, however, that the small scale resolution of the HICAT data was close to 50 m.

Next, in order to use Eq. (18) to obtain  $K_H(k_1)$  from  $\epsilon$ , they had to obtain values for  $a$  and  $C_1$ . For this purpose they used  $a = 0.5$  (based upon published experimental values). For  $C_1$  they used 0.51 which was derived in an appendix by Pao and Zimmerman.<sup>41</sup> This appendix used data published by Kellogg<sup>42</sup> and used the assumption that Eulerian and Lagrangian diffusion were approximately equal. In this way they arrived at  $K_H \sim (\epsilon/N_B^2)(2.77)^{-1}$  which is, for present purposes, identical to Eq. (12).\*

Three specific runs from the HICAT data were employed: 264 run 18, 233 run 3, and 280 run 10. They obtained values of  $K_H$  ranging from  $2.8 \times 10^5 \text{ cm}^2/\text{s}$  to  $37.5 \times 10^5 \text{ cm}^2/\text{s}$  which corresponded to a range of  $\epsilon$  from  $24 \text{ cm}^2/\text{s}^3$  to  $92 \text{ cm}^2/\text{s}^3$ . The largest value of  $\epsilon$  was listed as  $262 \text{ cm}^2/\text{s}$ . These should be compared to values found by Lilly et al,<sup>15, 16</sup> where  $\epsilon$  (mean dissipation rates) were in the range  $2.82 \text{ cm}^2/\text{s}^3$  to  $20.0 \text{ cm}^2/\text{s}^3$ ; and  $K_H$  values given in tables were

\*This observation was originally made by Dr. R. E. Good in private conversation.

39. Lumley, J. L. (1965) Theoretical Aspects of Research on Turbulence in Stratified Flows—Atmospheric Turbulence and Radio-wave Propagation, International Colloquium, Moscow:105-112.
40. Shur, G. (1962) Experimental Studies of the Energy Spectrum of Atmospheric Turbulence, Proceedings of Central Aerological Observatory, 43.
41. Pao, M. and Zimmerman, S. (1975) Lagrangian description of turbulent diffusion, The Natural Stratosphere of 1974, CIAP Monograph 1, Final Report, DOT-TST-75-51:6-123-6-124.
42. Kellogg, W. (1956) Diffusion of smoke in the stratosphere, Journal of Meteorology 13:241.

In the range of  $2600 \text{ cm}^2/\text{s}$  to  $12,200 \text{ cm}^2/\text{s}$ . We see that reasonable men can differ by two orders of magnitude when it comes to stratospheric diffusion estimates. When  $K_H$  estimates as obtained by Zimmerman et al<sup>23</sup> are reduced by a factor of 100, they fall into the range  $0.3$  to  $3.0 \text{ m}^2/\text{s}$  which is the same range reported in the literature for all transport mechanisms for stratospheric transport.<sup>19-21</sup>

One particularly disturbing aspect of their treatment is that the  $k^{-3}$  dependence of the velocity power density spectrum (predicted by the same theory that they used to calculate  $k_1$  which in turn was used in Eq. (18) to obtain  $K_H$ ) is in blatant contradiction to the HICAT data upon which they base their estimates. They did not overlook this fact and they warned the reader about it. They did not, however, give an argument as to why this would not invalidate their estimate and, therefore, we are left with this as an unanswered question. In any case, if their results were in fact of the correct order of magnitude, such results would be considered as evidence that turbulence plays a significant role in vertical stratospheric transport.\*

## 5. FURTHER CRITICAL REMARKS APPLICABLE TO ALL THE ABOVE TREATMENTS

### 5.1 The -5/3 Spectra and the Determination of $\epsilon$

In all of the estimates of  $K_H$  described to this point, the assumption was made that the HICAT spectra were in the inertial range. The assumption seems, however, to be not valid for the reasons cited previously. On the other hand, the possibility exists that in spite of this these spectra might be analogous to inertial range spectra to some degree of approximation.

A simple, theoretical explanation of the HICAT -5/3 spectra was proposed by Dewan<sup>9, 10, 43</sup> which assumes that these spectra are mostly due to waves and that the slope is due to an energy cascade caused by the small nonlinear interactions between the waves of various scales. This wave cascade was presumed to be the source of the energy which eventually finds its way to the turbulence cascades inside the blind. Thus, in fact, if this theory were correct, it implies that indeed there is a physical basis for the existence of spectra at large scales which are

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\*S. Zimmerman, in a recent private communication, revised  $K_B$  down to  $0.1 \text{ m}^2/\text{s}$  on the basis of statistics not previously considered. The conclusion remains unaltered.

43. Dewan, E.M. (1979) Mixing in Billow Turbulence and Stratospheric Eddy Diffusion, AFGL-TR-79-0091, AD A074 466.

analogous to inertial range spectra.\* This raises the next question: Would the value of  $\epsilon$  derived from such spectra be even approximately equal to the value of  $\epsilon$  found in a valid manner (that is, from an actual inertial range spectrum)?

To answer this question we consider the hypothetical relation between the wave-cascade spectrum,  $S_W(k)$ , and the inertial turbulence-cascade spectrum,  $S_T(k)$ :

$$S_W(k) = a_W \epsilon_W^{2/3} k_W^{-5/3} \quad (20)$$

from Dewan<sup>8, 10, 43</sup> and

$$S_T(k) = a_T \epsilon_T^{2/3} k_T^{-5/3} \quad (21)$$

where subscripts W and T are used to designate wave and turbulence quantities. While  $a_T$  is known, all that one knows about  $a_W$  is that it is of order unity. According to Bond,<sup>48</sup> we do know a little more. He has demonstrated that constants like  $a_W$  are either greater than 5 or less than 1/5 in 1/5 of all cases studied by him and that the probability is 1/10 that it be greater than 10 or less than 10, etc. For convenience in what follows, we shall set  $a_W = a_T$  and try to relate  $\epsilon_T$  to  $\epsilon_W$ .

Let us assume that the flux of energy from large scale (over 10 km) to small scale (less than 1 m) is conserved. In other words, we assume that there are no energy sources or sinks at intermediate scale. This would be a dubious assumption in the case of boundary layer turbulence since heating of the ground can cause buoyancy driven turbulence of intermediate scales. We assume, however, that Eqs. (20) and (21) represent parts of a conservative cascade. This leads directly to the following expression of flux conservation (down the scale):

\*One other possibly viable suggestion has been proposed to explain the type of spectra seen in the HICAT results.<sup>8, 44</sup> This explanation involves the "two dimensional" turbulence cascade which according to Kraichnan,<sup>45</sup> gives a  $k^{-5/3}$  spectrum (one-dimensional) associated with a cascade going in the reverse direction; that is, from small to large scales. The explanation leaves unanswered three questions: Why does the two-dimensional turbulence exist in layers of order 1 km in thickness?<sup>22</sup> (This is easily explained in terms of trapped gravity waves on the other hand.) Also, what is the source of energy at small scales (less than 50 m)? If the answer is "turbulence," then, what is the source for the turbulence energy?

- 44. Gage, K. S. (1979) Evidence for a  $k^{-5/3}$  low inertial range in mesoscale two-dimensional turbulence, *J. Atm. Sci.* 36:1950-1954.
- 45. Kraichnan, R. H. (1967) *Physics of Fluids* 10:1417.
- 46. Bond, W. N. (1929) *Phil. Mag.* 7:722.

$$D_W V_W = D_T V_T \quad (22)$$

where  $D_W$  is the energy dissipation rate in the wave cascade per unit volume,  $D_T$  the same quantity for the turbulence cascade, and  $V_W$  and  $V_T$  the volume occupied by the waves and turbulence respectively. That these volumes can differ significantly was demonstrated by the observations of Woods,<sup>2</sup> and Woods and Wiley.<sup>3</sup> They saw internal waves in the upper ocean perturbing relatively rare and thin turbulent layers and yielding their energy (originally spread over both laminar and turbulent layers alike) to the turbulence cascades via the Kelvin-Helmholtz instability. Now

$$D_W = \rho \epsilon_W \text{ and } D_T = \rho \epsilon_T \quad (23)$$

where  $\rho$  is the mass density. Thus, from Eq. (22)

$$(\rho \epsilon_W) V_W = (\rho \epsilon_T) V_T \quad (24)$$

$$\epsilon_T = \epsilon_W \left( \frac{V_W}{V_T} \right) \quad (25)^*$$

Thus, if  $V_W \gg V_T$ , which is to be expected, then  $\epsilon_T \gg \epsilon_W$  and  $K_H$  would have to be altered accordingly (assuming that  $K_H \sim \epsilon$  were valid).

An important but unanswered question in connection with the above argument is "are the HICAT spectra due mostly to waves?" This could be determined by means of the appropriate cross spectra, but such has not yet been done in the literature. Due account would also have to be taken of aliasing effects in such a study. But in spite of such unanswered questions, it is now possible to state with certainty that it is indeed possible to make a significant error in the estimation of  $\epsilon$  directly from -5/3 spectra when in fact the latter represent waves instead of turbulence as is most likely the case for HICAT data. This would imply, for example, that, assuming  $K_H \sim \epsilon/N_B^2$  were valid, the results of Lilly et al.,<sup>15,16</sup> are too low by the factor  $(V_W/V_T)$ .

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\*These considerations were not included in Dewan.<sup>9, 10, 43</sup>

## 5.2 Extreme Inhomogeneity and Its Effect Upon the Relationship Between $K_B$ and $K_H$

In the following it will be shown that there are conditions such that  $K_H$  would have no influence upon the value of  $K_B$  and that such conditions are not at all unlikely in the case of stratospheric turbulence (or stratified turbulence in general). As in Section 5.1, all the above methods to estimate  $K_B$  will be affected by the criticisms raised there. Eq. (13) is repeated here for convenience,

$$K_B = F K_H \quad . \quad (13)$$

In other words, one multiplies the eddy diffusion coefficient corresponding to the turbulence effects within an actively mixing layer by the fraction of the vertical dimension which is turbulent in order to estimate the bulk transport in that direction in terms of a diffusion parameter. One "dilutes"  $K_H$  to estimate  $K_B$ . To almost anyone this sounds reasonable at first. Eq. (13), however, is not always valid. The layered turbulence structure (blini) in the stratosphere has already been described; and these rare, active, mixing layers separated by laminar flow bear no resemblance whatever to homogeneous turbulence. Usually homogeneity assumptions are made in turbulence theory, but it should be clear that in the present case any such assumption would be manifestly invalid. We shall examine below some special examples where Eq. (13) is clearly invalid so that the main difficulties associated with it will be put in evidence.

First, it is necessary to define  $K_B$  as explicitly as possible. Figure 1 shows a slab of atmosphere much thicker than a turbulent layer. Let a scalar constituent (temperature, neutrally buoyant trace gas, etc.) be given by  $\bar{\psi}(Z)$  and let A and B be points at the top and bottom of the slab as indicated. We define  $K_B$  by

$$K_B = \frac{\text{FLUX (from A to B)}}{(\partial \bar{\psi} / \partial Z)} \quad (26)$$

where, from A to B the gradient  $\partial \bar{\psi} / \partial Z$  can be taken as constant.

We now consider the case where there is a single active layer of turbulence located at altitude C, and this layer is presumed to be permanently fixed for all time. For example, let the distance from A to B be 10 km and the layer thickness be 200 m. According to Eq. (13),  $K_B = K_H (200 \text{ m} / 10^4 \text{ m})$ . Is this correct? Consider Eq. (26). Since there is obviously no flux from point A to point B taking place (Figure 1), one must conclude that in the case considered,  $K_B = 0$ . (Molecular transport is, of course, being omitted from the discussion there.)

\*The first part of Section 5.2 is a brief summary of part of Dewan.<sup>12</sup>

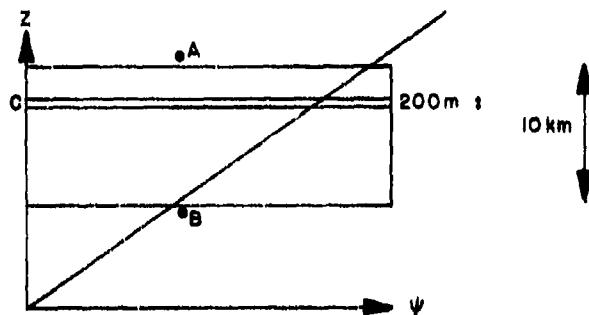


Figure 1. Single Turbulent Layer in Slab of Fluid Extending Between Altitudes A and B. The  $\psi$  denotes the mixing ratio of pollutant

The above situation resembles the case in electrical conductivity where one has a "sandwich" consisting of a horizontal slab of insulation above and below a horizontal slab of conductor as shown in Figure 2. In this configuration there is no way that current can flow vertically as indicated.

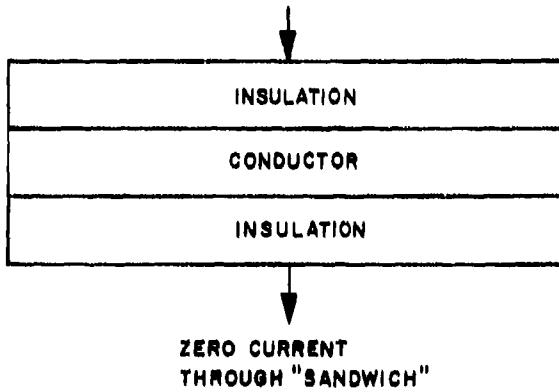


Figure 2. Insulator-Conductor-Insulator Sandwich Analogy for Transport by Layered Turbulence

Next, consider Figure 3 which depicts a more realistic situation where the single fixed layer is replaced by an ensemble of randomly spaced layers. Their thicknesses are random as well as their spacing. Total mixing is presumed to take place within the turbulent layers. If we assume, as before, that these layers remain at fixed altitudes for the entire duration of the experiment,

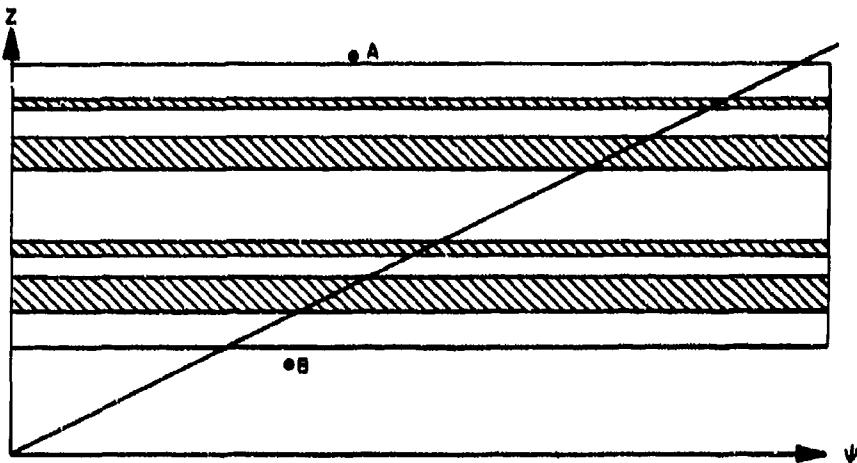


Figure 3. The Case of Many Layers of Turbulence in Fluid Slab (A to B)

then, again, we must conclude that  $K_B = 0$  (since again there is no transport from point A to point B). On the other hand, let us next suppose that the layers do not maintain a constant configuration but, instead, have a finite duration and are replaced by other layers at random heights and thicknesses. After a sufficient length of time has elapsed, all altitudes will have been covered by turbulent layers several times. Thus, when enough time has passed there would indeed be a flux from A to B. This type of flux has been studied in detail in Dewan<sup>11</sup> by means of computer simulations. The flow over short times will be very irregular; but over long times and with stationary statistical behavior of layer formation, the average flow becomes constant. Thus  $K_B$  could be directly "measured" by means of Eq. (26).<sup>12</sup>

Let  $\Delta t$  be the time interval between different layer configurations. In other words, the time between the commencement of one entire layer ensemble (Figure 3) and the event when it is replaced by a new one is  $\Delta t$ . If  $\Delta t$  were doubled, what would happen to the flux going from A to B? It would, of course, be halved, and hence  $K_B$  would be reduced by a factor of 2. From this we see that  $K_B \sim (\Delta t)^{-1}$ . In what way will  $K_B$  depend upon  $\Lambda$ , the average layer thickness? Also, how would  $K_B$  depend upon  $F$ , the fraction of vertical dimension turbulent? Since these questions have been treated elsewhere,<sup>11, 12</sup> and since the answer can be obtained by using Eq. (26) directly, we merely state the final result,<sup>8, 9, 10, 43</sup>

$$K_B = \frac{\gamma F \Lambda^2}{\Delta t} \quad (27)$$

Here  $\gamma \sim (1/10)$ . The most important thing to notice in Eq. (27) is that  $K_H$  (or  $\epsilon$ ) is not relevant. Only the layer thickness,  $\Lambda$ , "cycle time,"  $\Delta t$ , and  $F$  play any role in the flux in Figure 3 (A to B). The assumption that nearly total mixing takes place is the reason that  $K_H$  drops out of the argument in this inhomogeneous situation. Thus, under such conditions one must rule out Eq. (13) for  $K_B$ .

The parameter,  $K_B$ , given by Eq. (27) is not an eddy diffusion coefficient of the usual type. Eddy diffusion is usually associated with the definition,

$$K = v' l' \quad (28)$$

where  $v'$  and  $l'$  are the velocity and length scales of homogeneous turbulence. To avoid confusion, therefore, perhaps one should call  $K_B$  in Eq. (27) the "stratified turbulence diffusion parameter."

Does nearly total mixing take place in the stratosphere? This, of course, is one of the most important questions in connection with the use of Eq. (27). Mantis and Peplin<sup>47</sup> measured temperature profiles in the upper troposphere and lower stratosphere by means of balloon-borne sensors. Their observations are consistent with the hypothesis that nearly total mixing occurs, because they found nearly adiabatic lapse rates over regions of order 200 m thick which numerically is the same thickness to be expected of typical turbulence layers (a "coincidence" not to be taken lightly). The only alternative explanation I can find is that the adiabatic lapse rate regions may actually be unrelated to turbulence but be actually due to intrusions of layers of air (at particular altitudes) which are premixed.\* Such an hypothesis while it would explain the observations would leave unanswered the questions of how and where such intrusion layers could be formed in the stratosphere.

Observations in the upper ocean<sup>2,3</sup> show temperature profiles which are shaped like "steps" (that is, have regions where temperature is constant with respect to depth). The size of these steps (in complete analogy with the observations of Mantis and Peplin<sup>47</sup>) is the same as the typical size of a turbulent layer in the ocean. Since there is some analogy between the dynamics of the upper ocean and stratosphere, one can regard this observation as indirect evidence of the total mixing assumption. On the other hand, the "intrusion layer" phenomenon is known to occur in the ocean.

While there exists published evidence which is consistent with the nearly total mixing assumption, and while there is numerical agreement between the vertical

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\* Koop in private communication.

47. Mantis, M. T. and Peplin, T. J. (1971) Vertical temperature structure of the free atmosphere at mesoscale, *J. Geophys. Res.*, 76:8621-8628.

thickness of mixed layers and the thicknesses of observed turbulent layers, this issue is not yet settled. In view of this, it is important to consider reasonable experiments which can be performed to obtain more definitive evidence. An especially attractive approach seems to be the technique used by Browning and Watkins<sup>48</sup> and Browning<sup>49</sup>. Under certain conditions the troposphere exhibits the same sort of blini as those seen in the stratosphere. These are called CAT (clear air turbulence) and appear on radar in a very well-defined manner. These authors used radar observations on tropospheric CAT layers in conjunction with balloon observations of temperature and wind shear. They were able to measure Richardson number profiles as a function of time by means of repeated balloon launchings. In Figure 3 of Browning and Watkins<sup>48</sup> the vertical gradient of potential temperature, the vertical shear of the horizontal winds, and the vertical profile of  $R_i$  are plotted before and after the occurrence of a billow event. This figure clearly shows that some mixing occurs, but if this were typical, it would indicate that the mixing is significantly far from total. Unfortunately, the resolution of the balloon measurements is about 200 m, and higher resolution would be very helpful. Also, if billows typically did not cause a large amount of mixing, there remains the question of whether or not a series of billows such as those seen in references 2 and 3 could in time cause nearly adiabatic lapse rates. If this were to be the case, then one would have to estimate  $\Delta t$  with this in mind, and it appears likely that  $\Delta t$  would then be significantly more difficult to estimate than otherwise (see below). The effect would be to increase  $\Delta t$  and decrease  $K_B$ .

Alternatively, if much less than total mixing were to occur, Eq. (27) could be modified to take this into account. Such possible modifications will be discussed elsewhere; however, for sufficiently small mixing, it has been shown<sup>8, 10, 43</sup> that Eq. (27) for  $K_B$  would have to be replaced by Eq. (13). Thus, there are conditions where Eq. (13) is actually valid. To use Eq. (13), however, as Lilly et al<sup>15, 16</sup> and Heck et al<sup>17</sup> have done, leaves us with unanswered questions pertaining to its validity. First, one must obtain valid experimental evidence of the actual degree of mixing in stratospheric turbulent layers. In a word, perhaps one of the most important single experiments to perform at this time regarding stratified turbulence would be a repetition of the work of Browning and Watkins<sup>47</sup> with higher resolution and over longer periods of continuous observations.

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48. Browning, K. A. and Watkins, C. D. (1970) Observations of clear air turbulence by high power radar, Nature 227:260-263.
  49. Browning, K. A. (1971) Structure of the atmosphere in the vicinity of large amplitude Kelvin-Helmholtz billows, Roy. Met. Soc. Quart. J. 97:283-299.

## 6. THE WORK OF ROSENBERG AND DEWAN

Equation (27) was first derived and applied by Rosenberg and Dewan.<sup>8</sup> It was applied to a data base consisting of 200 vertical profiles of horizontal winds originally obtained by Miller et al<sup>50</sup> of NASA. These wind profiles were obtained by sending up rockets which left behind long trails of smoke which were approximately vertical. These smoke trails were then photographed from three widely spaced locations in simultaneous time-lapsed fashion. The series of photographs were used to estimate wind by means of triangulation: The location of points along a trail could be obtained in three dimensions as a function of time, and this gave the horizontal wind profile. The altitude range studied was 12 to 18 km, and, thus, it included both upper troposphere and lower stratosphere. Miller and Henry et al,<sup>50</sup> provided the data at a vertical resolution of 25 m.

In order to estimate the turbulent transport effects, Rosenberg and Dewan<sup>8</sup> first obtained vertical profiles of the Richardson number. The main idea was to ascertain which altitudes had  $R_i < 1/4$ , and, hence, estimate the probability of occurrence and probable thicknesses ( $F$  and  $A$  in Eq. (27)) of the turbulent layers. Unfortunately, no simultaneous temperature profiles were available in connection with the wind profiles, and, hence, in order to obtain the Richardson numbers (which, of course, depend on the potential temperature gradients), we had to resort to a model atmosphere. This raises the as yet unanswered question: How much would the results be altered if actual rather than assumed temperature profiles were employed? Our laboratory has conducted experiments which should help to provide an answer to this question in the future.

A second unanswered question relating to this work involves the fact that sometimes  $R_i$  could go below 1/4 and then return subsequently to a value above 1/4 without an intervening billow event. This effect would have to be included in accurate assessments of  $K_B$ . The work of Browning<sup>49</sup> shows that this does indeed happen sometimes.

This work took into account the effect of turbulent spreading. As has already been mentioned, the conclusion was that  $K_B \sim 0.3 \text{ m}^2/\text{s}$  which would make turbulence of the small scale variety one of the significant agents in the vertical transport process within the stratosphere. Total mixing within the layers was assumed, of course, and this, as was already emphasized, must be further tested. But there is yet one more assumption made, not only by Rosenberg and Dewan,<sup>8</sup> but by all the previously cited authors, which if not correct, could wreak havoc with

50. Miller, R.W., Henry, R.M., and Rowe, M.G. (1965) Wind Velocity Profiles Measured by the Smoke-Trail Method at Wallops Island, Virginia. NASA TN D-2937 (1959-1962), see also NASA TN D-4365 (1966).

all  $K_B$  estimates. This is the assumption that turbulent layers form at random altitudes.

The postulate of the randomness of altitude for turbulent layer formation has the following justifications: (1) It is not in contradiction with known data,\* and (2) it is the simplest hypothesis to make in the face of our present ignorance. On the other hand, it still remains entirely possible that turbulent layers have "preferred altitudes" over long durations of time relative to the time history of layer creation and subsequent decay. Such a possibility is, in fact, implicit in the previously cited theory, Dewan,<sup>8, 10, 43</sup> for the explanation of the HICAT data. In the latter theory it is assumed that project HICAT measured trapped gravity waves. Trapped gravity waves could certainly give rise to preferred altitude regions of turbulent layer formations.

The best technique for testing the random altitude assumption would be one which uses high powered radar to detect stratospheric turbulence at one geographic location over extended periods of time. Such observations have been made by Crane,<sup>26, 51</sup> VanZandt et al,<sup>52</sup> Woodman,<sup>53</sup> and Watkins.<sup>54</sup> Unfortunately, in all but one of these observations (Woodman's, with resolution of 150 m) the highest resolution is of order 1 km. Since the expected layer thickness is of order 100 m, the resolution must be, in general, greatly improved and more extensive observations must be made. Since this assumption of randomness is crucial for the theory (that is,  $K_B$  could = 0 as we have already seen in Figure 1), such experiments have very high priority if not the highest priority with regard to turbulence transport.

In Rosenberg and Dewan<sup>8</sup> the symbol "L" was used for "layer thickness." Later on it became evident that, in actual fact, L was a "half-thickness," that is,  $L = (\Lambda/2)$ . In view of this, we found that  $\Lambda$  typically was of order 200 m. Turbulent layers of such size were measured by Barat,<sup>28</sup> Crane,<sup>26, 51</sup> and Cadet,<sup>27</sup> by means of in-situ measurements. These measurements lend credibility to the value of  $\Lambda$  which were reported in Rosenberg and Dewan,<sup>8</sup> even though we subsequently learned that, at 25 m resolution there was a large error in velocity (of order 0.1 m/s to 1.0 m/s). Further assurance came from the fact that an estimate of  $\Lambda$  based on a smooth velocity profile of 100 m resolution resulted in

\*Ignore here the observation by Crane<sup>22</sup> that there seems to be a "persistent layer at the tropopause," (compare his Figure 32). The reason is that we are considering transport throughout the body of the stratosphere and not the boundary effects.

51. Crane, R. K. (1980) A review of radar observations of turbulence in the lower stratosphere, Radio Science 15:177-193.
52. VanZandt, T. E., Green, J. L., Gage, K. S., and Clark, W. L. (1978) Vertical profiles of refractivity turbulence structure constant, Radio Sci. 13:819-829.
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$K_B = 0.24 \text{ m}^2/\text{s}$  instead of  $0.34 \text{ m}^2/\text{s}$  for 25 m resolution. Such a small reduction in  $K_B$  did not alter our conclusion, and the 100 m spacing greatly diminished the spurious effects due to the "error" in velocity previously mentioned. Further treatment of this situation will be given elsewhere.

In Rosenberg and Dewan,<sup>8</sup> (Figure 5 in that paper) we found that, at 25 m resolution and 100 m resolution, the fraction  $F$  of turbulence is of the order of a few percent (for  $L \geq 25 \text{ m}$ ). This fraction of turbulence is numerically of the same general size found from the HICAT data. It is tempting to regard this as a form of mutual confirmation; however, caution is needed here. The HICAT data seem to have been primarily associated with gravity waves. In addition, they were taken over nearly horizontal trajectories. In contrast, the  $F$  in our work involved essentially vertical trajectories and involved "potential turbulence," that is, regions where  $R_1 < 1/4$  together with the estimated effects of spreading. That  $F$  is about equal in the two cases may thus be due to coincidence. In any case, the  $F$  derived by means of vertical profiles is the only one that is relevant so far as vertical  $K_B$  is concerned (Eq. (27)).

Another parameter in Eq. (27) is  $\Delta t$ . In Rosenberg and Dewan<sup>8</sup> the value of  $\Delta t$  was estimated from the previously cited work of Browning.<sup>49</sup> We found that, on average, 3000 sec would elapse between the time  $R_1 < 1/4$ , according to the balloon observations and the billow event (turbulence) according to the radar observation. Since these observations were carried out in the troposphere, it was, of course, necessary to make allowance for the change of the dynamical situation in the stratosphere, and, hence,  $\Delta t$  there was taken as 1500 sec (see the report for details of the argument). I feel that this  $\Delta t$  is accurate enough to estimate  $K_B$  "within one-half an order of magnitude" if the assumptions are correct. However, only after the key experiments where degree of mixing and randomness of altitude for turbulence have been performed would it make any sense at all to concentrate on the task of obtaining more accurate estimates for  $\Delta t$ .

One more critical remark should be made regarding the estimate of  $K_B$  in Rosenberg and Dewan.<sup>8</sup> It was based entirely upon data obtained from a single geographic location, namely, Wallops Island, Virginia. In addition, the data were obtained only on clear days. It would be much better if at least the geographical effects could be included since, as Lilly et al.,<sup>15,16</sup> have demonstrated, velocity fluctuations change significantly with changes in the shape of the terrain beneath.

## 7. CONCLUSION

We have seen that  $K_B$ , the bulk vertical transport parameter for small scale turbulence has been estimated to be of order  $0.01$ ,<sup>15,16</sup>  $0.10$ ,<sup>8</sup> and  $1.0$  or even

higher.<sup>23\*</sup> The question is which, if any, of these estimates is the one to be taken seriously? The higher values of Zimmerman and Loving<sup>23</sup> and Rosenberg and Dewan<sup>8</sup> would imply that turbulence plays a significant role in global vertical transport. The value published by Lilly et al.,<sup>15,16</sup> would imply an insignificant role.

We have seen that, without exception, all these estimates involve assumptions and unanswered questions which can only be tested and resolved by means of experiments that have yet to be adequately performed. In my opinion it is clear that, on the basis of the wide range of these independent estimates, and the unanswered questions associated with their derivations the most valid conclusion is that at present no available value of  $K_B$  is to be regarded as being conclusive or reliable. All we really know is that it cannot be larger than the values obtained from the fallout of tracers from the stratosphere which represent all the processes operating in concert.

But now let us bring in one more element. Mahlman and Moxim in a paper entitled, "Tracer Simulation Using a Global General Circulation Model: Results From a Mid-latitude Instantaneous Source Experiment" discussed the significance of the role of what they called "vertical subgrid-scale diffusion," (p. 1349).<sup>18</sup> In order to obtain an upper bound on  $K_B$  they inserted values of  $0.1 \text{ m}^2/\text{s}$  and  $0.5 \text{ m}^2/\text{s}$  into their "Global Circulation Model." They state their results as follows: "In both cases, the tracer transport from the stratosphere to the troposphere was drastically overestimated compared with observed behavior of radioactive tracers . . . . The above result suggests that subgrid-scale motions are considerably less important than large-scale motions in affecting stratospheric vertical tracer transfer. This inference is strengthened by analysis of Project HICAT spectra (for example, Lilly et al.),"<sup>18</sup>

As we have seen, there are too many unanswered questions in the work cited by Mahlman and Moxim<sup>18</sup> to lend much strength to their inference; and perhaps it is better to regard their result as standing solely on its own merits. In any case, they rightly consider it to be desirable to have an independent way to ascertain the value of  $K_B$ . This would, therefore, reinforce the need to perform the key experiments mentioned in the text regarding degree of mixing and randomness of altitude of formation. If indeed the upper limit of  $K_B$  inferred by Mahlman and Moxim,<sup>18</sup> eventually received valid independent support, then the role of small scale turbulence would be at last established. On the other hand, it would still be important to obtain more than the upper bound. The value of  $K_B$  remains important in the context of the chemistry of the stratosphere.

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\*But, as was mentioned, this has recently been revised downward to  $0.1 \text{ m}^2/\text{s}$  in a private communication.

Finally, I would like to state that our ignorance about  $K_B$  is much larger than may have seemed possible and that more work needs to be done in view of the importance of the problem.

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